which we find treated at considerable length. As to the arrangement of the chemical part, the method adopted in "Miller's Chemistry" of arranging the elements under the terms metals of the alkaline earths, &c., has been adopted, which is a very excellent method of arrangement for teaching purposes, as it allows of elements with similar properties being compared. There is evidently throughout the whole of the book a tendency to condense far too much into a small space. It would be an exceedingly difficult book indeed to be put before an *absolute* beginner. The explanatory part is reduced apparently as much as possible, although a great many facts are crammed in, certainly in good order; but still a beginner requires very much more explanation of facts than is to be found in this book. On that account, and being more an epitome of facts than explanations, especially in the chemical portion, it is scarcely possible to criticise it. The arrangement is very excellent and the details are well up to date. We notice that ozone has been put in in the form of an addendum: surely its position is closely in connection with oxygen. It is very liable in this position to be overlooked, or at any rate neglected, by a student. As there is such a considerable amount of attention given to the rare metals, especially vanadium, many of its compounds being detailed, it is somewhat surprising that davyum, though perhaps not yet absolutely settled, is not mentioned along with them. On looking carefully through the book, a number of points occur in which more explanation, or even an explanation of formulæ, would be very advantageous; but on the whole Mr. Watts is to be complimented on having produced a very complete, though certainly not quite elementary, manual on the science.

Arithmetical Chemistry. By C. J. Woodward, Birmingham and Midland Institute. (London: Simpkin, Marshall, and Co., 1884.)

THIS is almost a book of questions selected from the Cambridge and Oxford Local, University of London, Science and Art Department, and other examination papers. It is divided into headings on laboratory calculations, where, after an example of a volumetric or gravimetric analysis, a number of exercises and questions follows, and gas analysis with corrections of gases for pressure, &c., and determinations of vapour densities, specific and atomic volume, specific heat, calorific power, calorific intensity, kinetic theory of gases, and diffusion. The explanations are in most cases short and to the point, but the immense number of examples and exercises given tend to make it a "getting-up" book for examinations rather than a book to work with in the laboratory.

Experimental Chemistry. By J. Emerson Reynolds. Part III. (London: Longmans, Green, and Co., 1884.)

PROF. REYNOLDS, in the first and second parts of this little work, has departed somewhat from the usual method adopted in chemical books for junior students. The first and second parts deal entirely with the non-metals and their compounds, acids, &c.; while the third part is devoted to metals. It is divided into numbered experiments for the student to perform in rotation, and should be exceedingly valuable to medical and other students who have only a short time at disposal for practical chemistry. There is no attempt at systematic analysis, but the experiments are sufficiently logically arranged to enable a student who gives his attention to them to be able to perform any simple qualitative analysis. At the same time each experiment is very fully explained, and the reactions expressed in most cases with equations. Part III. is supplemented by a series of analytical tables at the end, which, however, are not very clear. They are certainly somewhat too complex for the class of student for which the book is intended. On the whole, however, it is a very excellent work.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Reply to Mr. Grubb's Criticisms on the Equatorial Coudé of the Paris Observatory

I HAVE just received the last number of the *Scientific Transactions of the Royal Society of Dublin*, containing a description of an instrument analogous to the equatorial constructed on a principle which I published in 1871 in the *Comptes Rendus*. Since it seems to me indispensable after I have studied Mr. Grubb's memoir to reply to it publicly, I beg you to insert this note in NATURE. In order to give greater importance to the modifications imagined by himself, the author, Mr. Grubb, submits the instrument imagined by me to a very severe criticism, and attributes very severe defects to it. There is no doubt that there is a considerable difference between the instruments in question. One of them, in fact, as experiments carried on during two years have proved, lends itself to the execution of all possible astronomical researches under the best possible conditions for securing precision, while the other, according to the description given in the publication above cited, renders impossible a very great part of the researches to which an equatorial is specially destined. Further on I shall insist on this difference, but I wish first of all to reply to Mr. Grubb's objections and to show that they are in all points contrary to the fact.

To give weight to his argument Mr. Grubb examines a case of the construction of an instrument of 27 inches aperture, and he anticipates in the construction the following difficulties, which he considers insurmountable:—(1) The optical difficulty of constructing a large plane mirror. (2) The practical difficulty of procuring a disk of the necessary dimensions. Mr. Grubb affirms that there is no glass-works capable of making a disk of glass so large. (3) The difficulty of moving a mirror of which the weight, according to Mr. Grubb's calculations, will be very nearly half a ton. (4) The dearness of the instrument, which would cost more than an ordinary equatorial, plus dome and observatory.

I will discuss these points one by one. (1) The construction of plane mirrors is a settled question nowadays. Many astronomers have been able to convince themselves that in the equatorial coudé of 11 inches aperture the introduction of a plane mirror of 16 inches in no way marred the definition. The brothers Henry, who constructed this mirror, have just made another of 40 inches diameter, which leaves absolutely nothing to be desired in this respect. This difficulty, moreover, is so little felt or feared by our opticians that the price of a similar mirror for an instrument coudé is only about a quarter of the price of an object-glass, in spite of the great difference of their respective surfaces. Thus, for an object-glass of 27 inches, the price of which is 70,000 francs, that of a plane mirror of 38 inches is 18,000 francs. For an object-glass of 40 inches, price 200,000 francs, the corresponding mirror of 58 inches only costs 40,000 francs. I admit, nevertheless, that in this respect Mr. Grubb may have had apprehensions. In the past, in fact, serious difficulties have been met with in the production of plane mirrors, but my own personal experience has enabled me to realise the real cause of this want of success. Up to the present time, to satisfy preconceived ideas it was believed that to establish rapidly an equilibrium of temperature it was necessary that the thickness of the mirror should be small. Then, under the influence of a tightening, however slight, or only a flexion, the mirrors were deformed unequally, and consequently produced an obvious alteration in the beauty of the images. The brothers Henry, studying the same question by different processes, have arrived at the same conclusions. In giving to the disk a sufficient thickness, the production of a plane surface is not more difficult than any optical surface whatever. The means of verification are so delicate that in a mirror of 40 inches diameter an error of 1/50,000 of a millimetre can easily be determined and eliminated. So if there be a sphericity in the mirror, the radius of curvature will have at least 1600 leagues, that is to say, about

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the radius of curvature of a bath of mercury, and in taking still greater pains it will be possible to go further and to reach an exactitude such that the radius of curvature might be twice as great. Under these conditions it is not possible, as one can readily see, that there can be any appreciable imperfection. The problem is therefore solved, as I said above.

(2) The impossibility of finding glass-works capable of furnishing large disks for mirrors, pointed out by Mr. Grubb, does not exist. We have in reserve at the Observatory a disk for a plane mirror of 1·22 m. diameter, weighing 650 kilos. The mirror just finished by the brothers Henry has a diameter of 40 inches, with a thickness of ·17 m.; this weighs 380 kilos. The glass-works of St. Gobain are prepared to furnish blocks weighing a ton, the diameter 58 inches, to anybody who will order them.

(3) The third point in Mr. Grubb's criticism has arisen from some error in calculation. Mr. Grubb makes me say that it is necessary to give the mirror a thickness equal to 1/5 of the diameter, and then making some calculations with this false datum, he finds that the mirror necessary for an equatorial coudé of 27 inches would weigh 8½ tonnes, and it is this result that misleads him. Here are the facts. I have never said that the thickness should be ·18 of the diameter. I have given ·18 as a *maximum*. Allow me to quote textually what I have said (*Journal de Physique*, August 1883): "Des recherches effectuées avec des miroirs de 0·08 m. m'ont démontré que pour prevenir dans un miroir toute déformation causée par la flexion ou un leger serrage, il faut que l'épaisseur du verre soit 0·18 du diamètre. Peut être avec des miroirs plus grands sera-t-il possible de réduire notablement cette épaisseur; en tout cas, la fraction 0·18 doit être considérée comme un maximum."

In my equatorial coudé for example, the mirror has a thickness equivalent to 1/6 of the diameter. In taking 1/7 for a mirror of 38 inches destined for an object-glass of 27 inches, one has still nothing to fear, for its weight would scarcely reach 250 kilos., that is to say, the half of the weight indicated by Mr. Grubb. For an instrument which weighs already between 8 and 9 tonnes, this addition of 250 kilos., or of 280 kilos. if we adopt the proportion of 1/6, is so small as not to be worth mentioning. From a mechanical point of view the displacement of the cylinder carrying mirrors of 250 kilos., or even a tonne, considering both the movement and the stability of the installation, does not offer the least difficulty. It is a mechanical problem so elementary that not only the first-class artists of all countries but even ordinary constructors would be able to solve it without any great effort of intelligence. I am only astonished that a man of Mr. Grubb's reputation should have stopped to consider such a detail.

(4) We now come to the question of expense. I am curious to know whence Mr. Grubb could have got his information, because it differs so absolutely from the facts. In spite of the addition of two supplementary mirrors, the price of the new instrument is less, or at all events not greater, than an ordinary one. The simplicity of the construction is such that the saving in the mechanical part covers the expense of the optical one. As I have already pointed out in the *Comptes Rendus* in 1871 and 1873, and in the *Journal de Physique* already quoted, the considerable expense of rotating domes, &c., is entirely avoided. I am greatly astonished, therefore, that Mr. Grubb makes on this point, so thoroughly studied, an objection so little founded. If he had, moreover, looked at the drawing which I published in the *Journal de Physique* of last year, he would have seen that it is almost identical with that which he has communicated to the Dublin Society, so far as the general arrangements for sheltering the observer and instrument are concerned. M. Gautier has been good enough to furnish me with the prices.

Dimensions Inches	Ordinary Equatorial Francs	Equatorial Coudé Francs
12	48,000	44,000
18	81,000	79,000
27	183,000	183,000

These prices are for instruments of the most complete kind.

The objects which I wished to attain were (1) to realise an instrument more stable than the ordinary ones, and to render possible the measurement of large angular distances; (2) to establish an arrangement which permits the astronomer to explore the whole sky and to regulate himself, with the most perfect convenience, all the movements of his apparatus; (3) to avoid the necessity of those monumental domes, of which both the building and the movement are always so costly; (4) to realise an instrument which, in spite of the introduction of two supplementary mirrors, would be optically more perfect. Indeed, being able to give the telescope a much greater focal distance than in the common large instruments, the achromatism may be rendered more perfect. I believe that I have succeeded in satisfying these various conditions, and that I can appeal to the judgment of several of the most eminent astronomers who have studied the instrument. Mr. Grubb makes a last reproach rather curiously. He is astonished that I have not as once applied the new system to the construction of a large instrument, for he adds, "No one finds any difficulty in working an equatorial of 11 inches." I do not know whether this opinion of Mr. Grubb is based on his personal experience, but I do know that all astronomers do not share his optimism on this point. To cite only one instance. In all the observatories where the discovery of comets or small planets is in question, telescopes of 6 or 7 inches aperture at the outside are employed. Continued work of this kind with a larger instrument is accompanied with fatigue. Now in my instrument, whatever be the nature of the research, a precious economy of time and energy is secured. If we have not in Paris a larger telescope on my plan, I must say that my regret is greater than that of Mr. Grubb, and that the lack of it does not depend on me.

In a subsequent letter I propose to discuss Mr. Grubb's counter proposal. M. LŒWY

Paris Observatory, April 10

On the Motion of Projectiles

EXPERIMENTS made in 1866 showed that the resistance of the air to the motion of projectiles varied only slightly for such forms of heads as were likely to be used in practice, and that it was of no importance whether the apex of the shot was pointed or rounded off.

From what I have said (NATURE, vol. xxix. p. 527) it is evident that before we can calculate the trajectory of shot we must know something of the quality of the gun from which the shot is supposed to be fired. For if trajectories of shot fired from the experimental guns were calculated by the use of the tabular values of K_v, it is plain that the calculated ranges would err in *excess* for the 3-, 7-, and 9-inch guns, and in *defect* for the 5-inch gun. As Mr. Ristori, using tabular coefficients, finds his calculated less than his experimental range, it may be that his gun gave a degree of steadiness above the average, as the 5-inch gun did. It is also necessary to be correctly informed of the exact *initial direction* taken by the shot, because, owing to the recoil of the gun, there is a "jump," which gives a greater elevation to the direction taken by the shot than the elevation of the axis of the gun. It is also a difficult matter to obtain the correct *initial velocity* of a small-arm bullet. For I am assured on the highest authority that the cotton threads of which my screens are composed, would be likely to cause unsteadiness in the motion of a small bullet, and if so, the measured velocity would be lower than the velocity of the undisturbed bullet. And if wire screens were used, the evil would be increased. From the excellent work done by Robins with a *small* ballistic pendulum, I am disposed to think that that instrument might be advantageously used in experiments with *small*-arm bullets.

As my tabular values of K_v were determined from the average results given by 3- to 9-inch shot, I did not venture to put them forward as being applicable to the calculation of the motion of small bullets. But recently Major McClintock, R.A., Assistant Superintendent Royal Small-Arms Factory, has used them for that purpose. He says:—"The accuracy of rifle-bullet trajectories calculated by means of Prof. Bashforth's tables, has been tested by firing a large number of rounds through paper screens placed at different points along the range. The rifle used in the experiment was the Martini-Henry, and the screens were erected at intervals along a 500-yards and a 1000-yards range. The result of the experiment was most satisfactory, the mean heights of the bullet-holes in the screens agreeing closely with the heights found by calculation."

As is well known, guns vary greatly in the degree of steadiness they impart to elongated shot, and this often changes with the initial velocity of the shot. It is important, therefore, to have a ready way of changing the tabular coefficients K_v in the ratio of $1 : \kappa$, when the coefficients will become κK_v, where κ is a constant for that round. In the calculation of the general tables, and in using the tables of values of integrals denoted by X, Y, and T, we have to deal with the product $\frac{d^2}{w} \times \left(\kappa K_v\right) =$

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